thereby causing the sudden production of large volumes of gases at the ordinary temperature.

In answer to Mr. Griffith's query, I may state that the two trees are 34 yards apart; that there is no other tree in a direct line between them, though there are two about 4 yards from this line, and about midway between them; that the trees are certainly not isolated in any way, since there are fifteen trees within less than 34 yards of one of them, and about the same number within the same distance of the other.

48 Bryanston Square. SPENCER PICKERING.

Yew Trees in Berks.

A COMMUNICATION from Mr. Walter Money respecting two yew trees which were planted in the churchyard of Basildon, Berks, by Charles, Lord Fane, in 1726, has appeared lately in some of the papers (*North Wilts Herald*, October 4, 1889, and *Standard*), in which he refers to the dimensions recorded in the parish register, taken in 1780 and 1796, and again by my father in 1834. He adds his own observations on the growth taken this year. As I happen to have the original notes made by the late Prof. J. S. Henslow, dated "1834, August," it may be not uninteresting to record them. He writes as follows:—

"Measurements of yew trees at Basildon Churchyard, planted in 1726; taken near the ground:

		Ft.	In.	
Tree to south, 1780	... ...	6	3	According to Register.
1796	... ...	8	6	
1834	... ...	8	9	[J. S. H.]
,,	(at 4 feet)	6	10½	
[1889	... ...	9	10	Mr. Money]

Tree to north [1780 and 1796 not recorded in register]:

			Ft.	In.	
Roots lately injured by digging graves.	1834	... ...	9	2½	[J. S. H.]
	,,	(at 4 feet)	6	9¾	
	[1889	... ...	9	6	Mr. Money]

"From the three observations of 1780, 1796, and 1834, it would appear that the period of rapid growth stopped about 1796; but it seems probable that the measurement here is somewhat too great compared with that of 1780, as well as with that of 1834; for

Growth to		Ft.	In.		Lines.		Lines.		Years.		Lines per ann.
1780	...	6	3	=	900	gives	300	diam. of gr. in	54,	*i.e.*	5½
1796	...	2	3	=	324	,,	108	,, ,,	16,	,,	6¾

"Allowing this measurement to be wrong by 6 inches, it will reduce it to a greater probability also with that of 1834; and we shall have—

Growth to		Ft.	In.		Lines.		Lines.		Years.		Lines per ann.
1796	...	1	9	=	252	gives	84	diam. of gr. in	16,	*i.e.*	5¼
1834	...		9	=	108	,,	36	,, ,,	38,	,,	1
[1889	...	1	1	=	156	,,	52	,, ,,	55,	,,	1]

"N.B.—The increase between 1780 and 1796 is too great, supposing the same parts to have been measured; and between 1796 and 1834 it is too little; therefore 1796 either took in too much of the circumference of the roots, or perhaps 1780 a little above them. Possibly the soil had become somewhat raised since 1796.

[Since 1834 the growth for the last fifty-five years will be seen to be exactly the same per annum, or 1 line.]

[With regard to the rate of increase at a height of 4 feet from the ground, he adds the following additional note.]

"Now the rate of increase of 4 feet from the ground is slower than that near the root, upon the whole, in the proportion of one-fourth, nearly. Taking, therefore, this fact with the indications given above, we may average the growth of the stem at 4 feet in the following way:—

"Diameter at 4 feet = $\frac{990}{3}$ lines = 330 (in 1834). Dividing this by the age, or 108 years, it gives 3 lines per annum nearly.

"Also 1780 to 1796 gives 84 lines for 16 years, *i.e.* 5 lines per annum.

"As it seems not to have grown much in the last twenty-eight years (*i.e.* up to 1834), if we allow 1 line for this period, and distribute for the eighty years of rapid growth, we get the following result; thus:—

First	20	years at	3½	lines	=	70	Young growth.
,,	40	,,	4	,,	=	160	
,,	20	,,	3	,,	=	60	
,,	28	,,	1½	,,	=	42	Settled period."

GEORGE HENSLOW.

Maxwell's "Electricity and Magnetism."

THERE is apparently a trifling slip in § 360 of Maxwell's "Electricity and Magnetism." The ratio of the resistance of pure iron at 100° C. to the resistance at 0° C. is there stated to be 1·645. This ratio is evidently calculated from the results given in Matthiesen's paper on the influence of temperature on the electric conducting power of thallium and iron (Proc. Roy. Soc., 1862–63). The true ratio for pure iron annealed in hydrogen is 1·6255. The other ratios mentioned in the paragraph are correctly deduced.

HERBERT TOMLINSON.

King's College, Strand, October 12.

AN EXAMINATION OF SOME POINTS IN PROF. WEISMANN'S THEORY OF HEREDITY.[1]

PROF. WEISMANN'S views on heredity and allied phenomena have met with such general acceptance that I feel it to be presumptuous on my part to attempt any criticism of them. I cannot but think, however, that a statement of the difficulties which they present to me, and of the inconsistencies which appear to exist in the argument, may be of value, not indeed as a refutation, but as drawing attention to those points which seem to require further elucidation.

It will be necessary for me to state Prof. Weismann's argument, and I shall endeavour, in so doing, to represent it as fully and as fairly as my apprehension of it will admit, and as far as possible in his own words. But this is a matter of no small difficulty, inasmuch as the argument has to be traced through a number of separate essays, even though these essays have been collected into one volume and translated into English. All the references which I make relate to the English edition.[2]

The fundamental fact upon which the whole argument is based, and which Prof. Weismann appears to have fully established, is that the body of unicellular organisms (monoplastides), as also that of undifferentiated multicellular organisms (homoplastides), is immortal, at any rate potentially. This position is clearly stated in the following passage (p. 25):—

> "The process of fission in the Amœba has been recently much discussed, and I am well aware that the life of the individual is generally believed to come to an end with the division which gives rise to two new individuals, as if death and reproduction were the same thing. But the process cannot truly be called death. Where is the dead body? What is it that dies? Nothing dies; the body of the animal only divides into two similar parts, possessing the same constitution."

Death is, on the contrary, a characteristic feature of differentiated multicellular organisms (heteroplastides); but even in these forms there is still an immortal part, for the reproductive cells which develop into new individuals are evidently as potentially immortal as the Amœba. In these higher organisms, therefore, the mortal cells are to be distinguished from the immortal. This distinction is drawn by Prof. Weismann as follows (p. 122):—

> "It is necessary to distinguish between the mortal and the immortal part of the individual—the body in its narrow sense (*soma*) and the germ-cells. Death only affects the former; the

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[1] This paper is an expansion of some remarks contributed to the discussion on "The Transmission of Acquired Characters," which took place in Section D during the recent meeting of the British Association at Newcastle.

[2] Weismann, "On Heredity" (Oxford: Clarendon Press, 1889).

germ-cells are potentially immortal, in so far as they are able, under favourable circumstances, to develop into a new individual, or, in other words, to surround themselves with a new body (*soma*)." (See also p. 158.)

This statement is further explained on p. 205:—

> "Strictly speaking, I have therefore fallen into an inaccuracy in maintaining (in former works) that the germ-cells are themselves immortal; they only contain the undying part of the organism—the germ-plasm; and although this substance is, as far as we know, invariably surrounded by a cell-body, it does not always control the latter, and thus confer upon it the character of a germ-cell."

Similarly, the substance of the body is termed somatoplasm (p. 104). Moreover, the germ-plasm is stated to be localized in the nucleus of the germ-cell (p. 179).

The first difficulty which presents itself is to understand how the mortal heteroplastides can have been evolved from the immortal monoplastides or homoplastides. The explanation which Prof. Weismann offers is as follows (p. 27):—

> "Let us now consider how it happened that multicellular animals and plants, which arose from unicellular forms of life, came to lose this power of living for ever.
>
> "The answer to this question is closely bound up with the principle of division of labour which appeared among multicellular organisms at a very early stage, and which has gradually led to the production of greater and greater complexity in their structure.
>
> "The first multicellular organism was probably a cluster of similar cells, but these units soon lost their original homogeneity. As the result of mere relative position, some of the cells were especially fitted to provide for the nutrition of the colony, while others undertook the work of reproduction. Hence the single group would come to be divided into two groups of cells, which may be called somatic and reproductive—the cells of the body as opposed to those which are concerned with reproduction. This differentiation was not at first absolute, and indeed it is not always so to-day. Among the lower Metazoa, such as the Polypes, the capacity for reproduction still exists to such a degree in the somatic cells, that a small number of them are able to give rise to a new organism—in fact, new individuals are normally produced by means of so-called buds. Furthermore, it is well known that many of the higher animals have retained considerable powers of regeneration; the salamander can replace its lost tail or foot, and the snail can reproduce its horns, eyes, &c.
>
> "As the complexity of the Metazoan body increased, the two groups of cells became more sharply separated from each other. Very soon the somatic cells surpassed the reproductive in number, and during this increase they became more and more broken up by the principle of division of labour into sharply separated systems of tissues. As these changes took place, the power of reproducing large parts of the organism was lost, while the power of reproducing the whole individual became concentrated in the reproductive cells alone."

It is clear that this explanation, plausible as it seems to be, leaves untouched the real question at issue; the question as to how mortal cells could have been evolved from immortal. Prof. Weismann himself seems to have been conscious of this, for on p. 139 he reverts to the subject as follows:—

> "It may be objected that cells of which the ancestors possessed the power of living for ever, could not have become potentially mortal (that is, subject to death from internal causes) either suddenly or gradually, for such a change would contradict the supposition which attributes immortality to their ancestors, and to the products of their division. This argument is valid, but it only applies so long as the descendants retain their original constitution. But as soon as the products of the fission of a potentially immortal cell acquire different constitutions by unequal fission, another possibility arises. Now it is conceivable that one of the products of fission might preserve the physical constitution necessary for immortality, but not the other, just as it is conceivable that such a cell—adapted for unending life—might bud off a small part, which would live a long time without the full capabilities of life possessed by the parent cell; again, it is possible that such a cell might extrude a certain portion of organic matter (a true excretion) which is already dead at the moment it leaves the body. Thus it is possible that true unequal cell-divisions, in which only one half possesses the condition necessary for increasing, may take place; and in the same way it is conceivable that the constitution of a cell determines the fixed duration of its life, examples of which are before us in the great number of cells in the higher Metazoa, which are destroyed by their functions. . . . But the reproductive cells cannot be limited in this way, and they alone are free from it. They could not lose their immortality, if indeed the Metazoa are derived from the immortal Protozoa, for from the very nature of that immortality it cannot be lost. From this point of view the body, or *soma*, appears in a certain sense as a secondary appendage of the real bearer of life—the reproductive cells."

Prof. Weismann here comes to closer quarters with the real question at issue, but still he does not fully face it. He invokes the principle of "unequal fission" to account for the acquisition of "different constitutions" by the products of fission, but he offers no explanation whatever of the *modus operandi* of unequal fission. He makes no suggestion as to the constitution of the body of the Protozoa; whether it consists, in his opinion, entirely of germ-plasm, and if not, whether or not the germ-plasm is localized in the nucleus. The only criticism which can be made is that the bare mention of "unequal fission" is not a sufficient answer to the objection "that cells, of which the ancestors possessed the power of living for ever, could not have become potentially mortal." It appears to me that any satisfactory answer to this objection must include the assumption that the immortal ancestors already contained a substance which was potentially mortal. It is impossible to conceive that unequal fission can take place in a cell consisting throughout of essentially the same kind of substance.

Very much the same difficulty presents itself in connection with the development of the embryo from the ovum or germ-cell; in the one case it is phylogenetic, in the other ontogenetic. Prof. Weismann goes into far greater detail in this latter case, and the statements which he makes concerning it may perhaps be intended to throw some light on the former.

The germ-cell, as pointed out above, is characterized by containing germ-plasm; and this germ-plasm is localized in the nucleus. There is one point which Prof. Weismann does not mention, and that is as to the nature of that portion of the germ-cell (including the cytoplasm and part of the nucleoplasm) which does not consist of germ-plasm. Of what, then, does it consist? It must consist of somatoplasm: there is no alternative. The germ-cell, then, consists mainly of mortal somatoplasm, and contains in its nucleus a certain amount of immortal germ-plasm. But, as shown in preceding quotations, Prof. Weismann holds that the whole germ-cell is immortal. In view of the constitution of the germ-cell, this view seems to be paradoxical, but it appears to be explained on the assumption that the substance of the nucleus determines the nature and character of the cell, though Prof. Weismann does not altogether commit himself to this assumption (see pp. 185, 205, 210).

From this point of view Prof. Weismann's suggestion that the development of mortal from immortal cells is due to unequal fission, seems to be quite intelligible, not only ontogenetically, but also phylogenetically, if we venture to assume that the constitution of a Protozoon is essentially the same as that of a germ-cell. It is easy to imagine that the nucleus of a Protozoon may be divided into two parts, one of which contains the whole or the greater part of the parental germ-plasm, the other containing none or only a small portion of it; the two resulting cells would be respectively immortal and mortal, and, supposing they remained coherent, would represent the reproductive and somatic portions of a heteroplastid body. Similarly, if such a division of the nucleus of the germ-cell took place, the two resulting cells would represent the reproductive and somatic portions of the body of the embryo.

But this does not appear to be Prof. Weismann's view of embryogeny. On the contrary, he holds strongly that the germ-plasm of the ovum gives rise, in part at least, to the somatoplasm of the embryo. Thus, on p. 168 he says:—

> "I have called this substance 'germ-plasm,' and have assumed that it possesses a highly complex structure, conferring upon it the power of developing into a complex organism. I have attempted to explain heredity by supposing that in each ontogeny a part of the specific germ-plasm contained in the parent egg-cell is not used up in the construction of the body of the offspring, but is preserved unchanged for the formation of the germ-cells of the following generation."

It is not a little remarkable that Prof. Weismann should not have offered any suggestion as to the conception which he has formed of the mode in which the conversion of germ-plasm into somatoplasm can take place, considering that this assumption is the key to his whole position. He has been at considerable pains to controvert the view that somatoplasm may be converted into germ-plasm; but in making this attack he has overlooked the necessity for defence. There is really no other criticism to be made on an unsupported assumption such as this, than to say that it involves a contradiction in terms. The idea of the conversion of germ-plasm into somatoplasm is quite as impossible as that of the conversion of somatoplasm into germ-plasm. It is absurd to say that an immortal substance can be converted into a mortal substance. If such an apparent change takes place, the only possible conclusion is that the so-called immortal substance was never truly immortal, inasmuch as it must have always possessed the potentiality of mortality.

It may perhaps be represented that the foregoing criticisms are altogether of too minute and detailed a character to affect the general validity of Prof. Weismann's argument. My answer is that I understand Prof. Weismann to imply that his theory of heredity is not—like, for instance, Darwin's theory of pangenesis—"a provisional or purely formal solution" (Weismann, p. 166) of the question, but one which is applicable to every detail of embryogeny, as well as to the more general phenomena of heredity and variation.

We may now proceed to the consideration of Prof. Weismann's theory of heredity. The essential features of it are given in the following paragraphs (p. 73):—

> "Among these unicellular organisms, heredity depends upon the continuity of the individual during the continual increase of its body by means of assimilation.
>
> "But how is it with the multicellular organisms which do not reproduce by means of simple division, and in which the whole body of the parent does not pass over into the offspring?
>
> "In such animals the power of reproduction is connected with certain cells, which, as germ-cells, may be contrasted with those which form the rest of the body; for the former have a totally different *rôle* to play; they are without significance for the life of the individual (that is, for the preservation of its life), and yet they alone possess the power of preserving the species. Each of these can, under certain conditions, develop into a complete organism of the same species as the parent, with every individual peculiarity of the latter reproduced more or less completely. How can such hereditary transmission of the characters of the parent take place? How can a single reproductive cell reproduce the whole body in all its details?"

Prof. Weismann's answer to these questions is as follows:—

> "We have an obvious means by which the inheritance of all transmitted peculiarities takes place, *in the continuity of the substance of the germ-cells, or germ-plasm*. If, as I believe, the substance of the germ-cells, the germ-plasm, has remained in perpetual continuity from the first origin of life, and if the germ-plasm and the substance of the body, the somatoplasm, have always occupied different spheres, and if changes in the latter only arise when they have been preceded by corresponding changes in the former, then we can, up to a certain point, understand the principle of heredity; or, at any rate, we can conceive that the human mind may at some time be capable of understanding it" (p. 104).
>
> "Now if it is impossible for the germ-cell to be, as it were, an extract of the whole body, and for all the cells of the organism to despatch small particles to the germ-cells, from which the latter derive the power of heredity; then there remain, as it seems to me, only two other possible, physiologically conceivable, theories as to the origin of germ-cells, manifesting such powers as we know they possess. Either the substance of the parent germ-cell is capable of undergoing a series of changes which, after the building-up of a new individual, leads back again to identical germ-cells; or the germ-cells are not derived at all, as far as their essential and characteristic substance i concerned, from the body of the individual, but they are derived directly from the parent germ-cell.
>
> "I believe that the latter view is the true one. . . . . propose to call it the theory of the 'continuity of the germ plasm,' for it is founded upon the idea that heredity is brough about by the transference, from one generation to another, of a substance with a definite chemical, and, above all, molecula constitution. I have called this substance 'germ-plasm,' and have assumed that it possesses a highly complex structure, conferring upon it the power of developing into a complex organism. I have attempted to explain heredity by supposing that in each ontogeny a part of the specific germ-plasm contained in the parent egg is not used up in the construction of the body of the offspring, but is reserved unchanged for the formation of the germ-cells of the following generation" (p. 167).
>
> "I believe that heredity depends upon the fact that a small portion of the effective substance of the germ, the germ-plasm, remains unchanged during the development of the ovum into an organism, and that this part of the germ-plasm serves as a foundation from which the germ-cells of the new organism are produced. There is, therefore, continuity of the germ-plasm from one generation to another. One might represent the germ-plasm by the metaphor of a long creeping root-stock from which plants arise at intervals, these latter representing the individuals of successive generations" (p. 266).

This theory appears to fully account for the transmission and maintenance of ancestral characters; but of course it depends on the assumption that the germ-plasm is a substance of great stability. This is, in fact, Prof. Weismann's view (p. 271):—

> "The germ-plasm, or idioplasm of the germ-cell (if this latter term be preferred), certainly possesses an exceedingly complex minute structure, but it is nevertheless a substance of extreme stability, for it absorbs nourishment, and grows enormously without the least change in its complex molecular structure."

In spite of the simple, and apparently satisfactory, explanation of the phenomena of heredity which this theory affords, there are, nevertheless, serious difficulties in the way of its acceptance. It is open to criticism even from Prof. Weismann's own standpoint. The fate of the germ-plasm of the fertilized ovum is, according to Prof. Weismann, to be converted in part into the somatoplasm of the embryo, and in part to be stored up in the germ-cells of the embryo. This being so, how are we to conceive that the germ-plasm of the ovum can impress upon the somatoplasm of the developing embryo the hereditary character of which it (the germ-plasm) is the bearer? This function cannot be discharged by that portion of the germ-plasm of the ovum which has become converted into the somatoplasm of the embryo, for the simple reason that it has ceased to be germ-plasm, and must therefore have lost the properties characteristic of that substance. Neither can it be discharged by that portion of the germ-plasm of the ovum which is aggregated in the germ-cells of the embryo, for under these cumstances it is withdrawn from all direct relation with the developing somatic cells. The question remains without an answer.

Still more is the theory open to criticism from the standpoint which I have established above. It is clear that the theory of the continuity of the germ-plasm, as explaining heredity, is only valid on the assumption that the germ-plasm of the ovum gives rise to the somatoplasm of the embryo. But I have shown above that the conversion of germ-plasm into somatoplasm is inconceivable; and, even if it be admitted, it cannot be seriously maintained that the whole body of the embryo is, in any case, developed solely from the germ-plasm of the ovum. On the contrary, since the embryo is developed from the *whole* of the nucleus and more or less of the cytoplasm of the ovum, it must be admitted that the non-germ-plasm, or somatoplasm, of the ovum provides a large part of the material in embryogeny. It is an obvious inference that, under these circumstances, hereditary characters may be transmitted from the parent to the offspring, not only by the germ-plasm, but also by the somatoplasm, of the ovum.

It might be replied to these criticisms that, even if it be admitted that germ-plasm cannot be converted into somatoplasm, and also that the somatic cells of the embryo are derived from the somatoplasm of the ovum, it is still conceivable that the nuclei of the somatic cells of the embryo may contain a certain amount of the germ-plasm of the ovum, not enough to confer upon the somatic cells the properties of germ-cells, but sufficient to determine their growth and differentiation in accordance with the hereditary tendencies of which the germ-plasm is the bearer. But this view does not appear to be held by Prof. Weismann, whose opinion with reference to the presence of germ-plasm in somatic cells is as follows (p. 211):—

> "I believe I have shown that theoretically hardly any objections can be raised against the view that the nuclear substance of somatic cells may contain unchanged germ-plasm, or that this germ-plasm may be transmitted along certain lines. It is true that we might imagine *a priori* that all somatic nuclei contain a small amount of unchanged germ-plasm. In Hydroids such an assumption cannot be made, because only certain cells in a certain succession possess the power of developing into germ-cells; but it might well be imagined that in some organisms it would be a great advantage if every part possessed the power of growing up into the whole organism, and of producing sexual cells under appropriate circumstances. Such cases might exist if it were possible for all somatic nuclei to contain a minute fraction of unchanged germ-plasm."

After alluding to the fact that new plants can be developed from leaves of Begonia which have been cut off and laid in moist sand, Prof. Weismann continues:—

> "But I think that this fact only proves that, in Begonia and similar plants, all the cells of the leaves, or perhaps only certain cells, contain a small amount of germ-plasm, and that, consequently, these plants are specially adapted for propagation by leaves. How is it, then, that all plants cannot be reproduced in this way? No one has ever grown a tree from a leaf of the lime or oak, or a flowering plant from the leaf of the tulip or convolvulus. It is insufficient to reply that, in the last-mentioned cases, the leaves are more strongly specialized, and have thus become unable to produce the germ-substance; for the leaf-cells in these different plants have hardly undergone histological differentiation in different degrees. If, notwithstanding, the one can produce a flowering plant, while the others have not the power, it is of course clear that reasons other than the degree of histological differentiation must exist; and, according to my opinion, such a reason is to be found in the admixture of a minute quantity of unchanged germ-plasm with some of their nuclei."

It appears, therefore, to be Prof. Weismann's opinion that it is only in special cases that germ-plasm is present in somatic cells, and that, when present, it confers on the somatic cells the properties of germ-cells, though it is difficult to reconcile this opinion with the following statement on p. 205:—

> "Strictly speaking, I have therefore fallen into an inaccuracy in maintaining (in former works) that the germ-cells are themselves immortal; they only contain the undying part of the organism—the germ-plasm; and although this substance is, as far as we know, invariably surrounded by a cell-body, it does not always control the latter, and thus confer upon it the character of a germ-cell."

I would submit, therefore, that, inasmuch as Prof. Weismann offers no evidence to prove the continuity of the germ-plasm of the ovum with the somatoplasm of the embryo, his principle of the continuity of the germ-plasm cannot be regarded as a satisfactory theory of heredity; and I would point out that the facts of embryogeny seem to confer upon the idea of a continuity of the somatoplasm at least as high a degree of probability as upon that of a continuity of the germ-plasm.

We come, finally, to Prof. Weismann's explanation of variation, a connected statement of which is to be found on pp. 277 *et seq.*, from which I may make the following quotations:—

> "The origin of hereditary individual variability cannot indeed be found in the higher organisms—the Metazoa and Metaphyta; but it is to be sought in the lowest—the unicellular organisms. In these latter the distinction between body-cell and germ-cell does not exist. Such organisms are reproduced by division, and if, therefore, any one of them becomes changed in the course of its life by some external influence, and thus receives an individual character, the method of reproduction insures that the acquired peculiarity will be transmitted to its descendants. If, for instance, a Protozoon, by constantly struggling against the influence of mechanical currents in water, were to gain a somewhat denser and more resistant protoplasm, or were to acquire the power of adhering more strongly than other individuals of its species, the peculiarity in question would be directly continued on into its two descendants, for the latter are at first nothing more than the two halves of the former. It therefore follows that every modification which appears in the course of its life, every individual character, however it may have arisen, must necessarily be directly transmitted to the two offspring of a unicellular organism" (p. 277).
>
> "We are thus driven to the conclusion that the ultimate origin of hereditary individual differences lies in the direct action of external influences upon the organism. Hereditary variability cannot, however, arise in this way at every stage of organic development, as biologists have hitherto been inclined to believe. It can only arise in the lowest unicellular organisms; and when once individual difference had been attained by these, it necessarily passed over into the higher organisms when they first appeared. Sexual reproduction coming into existence at the same time, the hereditary differences were increased and multiplied, and arranged in ever-changing combinations" (p. 279).
>
> "It is, however, obvious that sexual reproduction will readily afford such combinations of acquired characters, for by its means the most diverse features are continually united in the same individual, and this seems to me to be one of its most important results.
>
> "I do not know what meaning can be attributed to sexual reproduction other than the creation of hereditary individual characters to form the material upon which natural selection may work" (p. 281).

In the essay entitled "On the Number of Polar Bodies and their Significance in Heredity," Prof. Weismann explains his conception of the mode in which sexual reproduction promotes variability, showing, with the assistance of diagrams, how the nuclear germ-plasm of a fertilized ovum contains germ-plasms derived from the ancestors of both parents.

The conception of the process of variation which the preceding passages (as well as others) produce in the mind of the reader is that unicellular organisms acquired, during the period of their entirely asexual reproduction, a number of individual differences; and that, since the appearance of sexual reproduction, these ancestral characters have been combined in an infinite number of ways, leading to the evolution of all existing varieties of plants and animals, to say nothing of all the varieties which have perished in the struggle for existence. It would, in fact, appear that Prof. Weismann denies the acquisition of any new individual characters due to the influence of external conditions by any except unicellular organisms.

This being his view, we find, as might be expected, that Prof. Weismann opposes the assumption of the transmission, by means of sexual or amphigonic reproduction, of characters which he terms "somatogenic" (p. 413); that is, of characters which have manifested themselves in the *soma* of an individual, not spontaneously, but as the result of the operation of external forces or conditions; and he critically sifts the evidence for such transmission with results which, it must be admitted, tell in favour of his views.

For all that, Prof. Weismann does not take up an altogether uncompromising position with reference to this point; in fact, his statements of opinion are so fluctuating that it is difficult to determine what his position exactly is: witness the following quotations:—

> " . . . and it is impossible to imagine any way in which the transmission of changes, produced by the direct action of external forces upon the somatic cells, can be brought about" (p. 80).
>
> "Hence it follows that the transmission of acquired characters is an impossibility . . ." (p. 266).
>
> "For the germ-cells are contained in the organism, and the external influences which affect them are intimately connected with the state of the organism in which they lie hid. If it be well nourished, the germ-cells will have abundant nutriment; and, conversely, if it be weak and sickly, the germ-cells will be arrested in their growth. It is even possible that the effects of these influences may be more specialized; that is to say, they may act only upon certain parts of the germ-cells. But this is indeed very different from believing that the changes of the organism which result from external stimuli can be transmitted to the germ-cells, and will re-develop in the next generation at the same time as that at which they arose in the parent, and in the same part of the organism" (p. 103).
>
> "Still we cannot exclude the possibility of such a transmission occasionally occurring; for, even if the greater part of the effects must be attributed to natural selection, there might be a small part in certain cases which depends on this exceptional factor.
>
> "A complete and satisfactory refutation of such an opinion cannot be brought forward at present; we can only point out that such an assumption introduces new and entirely obscure forces, and that innumerable cases exist in which we can certainly exclude all assistance from the transmission of acquired characters" (p. 100).
>
> "If, on the other hand, acquired differences are transmitted, this would prove that there must be something wrong in the theory of the continuity of the germ-plasm, as above described, and in the non-transmission of acquired characters which results from this theory" (p. 268).
>
> "It seems to me that the problem of the transmission or non-transmission of acquired characters remains, whether the theory of the continuity of the germ-plasm be accepted or rejected" (p. 403).

I would remark, with reference to the statement that it is impossible to imagine any way in which somatogenic changes can be transmitted, that such a transmission is quite conceivable, and is even probable, when the continuity of the somatoplasm is borne in mind. If the ovum contains somatoplasm, as we are driven to assume, and if, as cannot be denied, the somatoplasm takes part in the formation of the body of the embryo, then it is not impossible that changes induced in the body of the parent, by the action of external conditions, may be transmitted to the offspring through the somatoplasm of the ovum. The discontinuity of the somatoplasm must be proved before the impossibility of the transmission of somatogenic characters can be considered to have been established.

But if Prof. Weismann is not prepared to admit that there is more than a remote possibility that variation may, in some degree, be due to the transmission of somatogenic characters, he makes a large concession in admitting that new characters may be acquired in another way, and, being transmissible, lead to variation. The first hint of this view is to be found on pp. 98, 99:—

> "These changes—such, for example, as are produced by a strange climate—have always been looked at under the supposition that they are transmitted and intensified from generation to generation, and for this reason the observations are not always sufficiently precise. It is difficult to say whether the changed climate may not first have changed the germ, and if this is the case the accumulation of effects through the action of heredity would present no difficulty" (p. 98).
>
> "It must be admitted that there are cases, such as the climatic varieties of certain butterflies, which raise some difficulties against this explanation. I myself, some years ago, experimentally investigated one such case, and even now I cannot explain the facts otherwise than by supposing the passive acquisition of characters produced by the direct influence of climate" (p. 99; see also above quotation from p. 103).

It is again mentioned on p. 271, but it is not prominently asserted until p. 410, where Prof. Weismann says:—

> "I have never doubted about the transmission of changes which depend upon an alteration in the germ-plasm of the reproductive cells, for I have always asserted that these changes, and these alone, must be transmitted. If anyone makes the contrary assertion, he merely proves that he does not understand what I have said upon the subject. In what other way could the transformation of species be produced, if changes in the germ-plasm cannot be transmitted? And how could the germ-plasm be changed except by the operation of external influences, using the words in their widest sense? . . ."

On pp. 402–403 Prof. Weismann defines his view more clearly:—

> "It is certainly necessary to have two terms which distinguish between two chief groups of characters—the primary characters which first appear in the body itself, and the secondary ones which owe their appearance to variations in the germ, however such variations may have arisen. We have hitherto been accustomed to call the former 'acquired characters,' but we might also call them *somatogenic*, because they follow from the reaction of the *soma* under external influences; while all other characters might be contrasted as *blastogenic*, because they include all those characters in the body which have arisen from changes in the germ. In this way we might perhaps prevent the possibility of misunderstanding. . . . Among the blastogenic characters, we include not only all the changes produced by natural selection operating upon variations in the germ, but all other characters which result from this latter cause."

The point is again mentioned on p. 433:—

> "It is therefore possible to imagine that the modifying effects of external influences upon the germ-plasm may be gradual and may increase in the course of generations, so that visible changes in the body (*soma*) are not produced until the effects have reached a certain intensity."

It is not a little remarkable that, after insisting so strongly, as in the passage previously quoted, on the extreme stability of the germ-plasm, Prof. Weismann should be prepared to admit that it is in so high a degree susceptible to the action of external influences. He is, however, inclined to complain, in the passage on p. 410, that this view of the production of blastogenic changes by external influences has been ignored; but the readers of the earlier essays may well be pardoned for inattention to this point, as it is only casually mentioned there, and is not put forward as an integral part of his theory of variation. No one reading the statement of his theory of variation on p. 277 would infer that Prof. Weismann attached any importance to the effect of external influences on the germ in producing new characters. In fact, Prof. Weismann himself seems hardly to realize how inevitable such a conclusion is. If it be admitted that unicellular organisms acquire new characters under the operation of external influences, it cannot consistently be doubted that this also takes place in germ-cells.

It seems to be necessary, therefore, to modify the conception of variation founded upon the above-quoted paragraph from p. 277, by introducing into it the operation of external influences upon the germ. We now see that though sexual reproduction greatly promotes variation in consequence of the ever-new combination of ancestral characters in each fertilization, yet another efficient cause of variation is the direct action of external influences on the germ, giving rise to blastogenic characters.

But this modified conception of the causes of variation comes into collision with Prof. Weismann's statement (p. 277) that "the origin of hereditary individual variability cannot indeed be found in the higher organisms—the Metazoa and Metaphyta; but it is to be sought for in the lowest—the unicellular organisms," a collision which is much to the detriment of the latter; for, if it cannot be denied that external influences give rise to blastogenic characters, then it cannot be maintained that "the origin of hereditary individual variability cannot be found in the higher organisms." On the contrary, it must be admitted that the modifying influence of external conditions continually affects not only unicellular organisms, but also the germ-cells of the Metazoa, producing new characters, thus inducing variation, in both.

This conclusion leads to the consideration of a point of great interest. In accordance with his view of the preeminent importance of amphigonic reproduction in causing variation, Prof. Weismann asserts the lack of variability in parthenogenetic forms, in the following words (p. 290):—

> "If my theory as to the causes of hereditary individual variability be correct, it follows that all species with purely parthenogenetic reproduction are sure to die out; not, indeed, because of any failure in meeting the existing conditions of life, but because they are incapable of transforming themselves into new species, or, in fact, of adapting themselves to any new conditions. Such species can no longer be subject to the process of natural selection, because, with the disappearance of sexual reproduction, they have also lost the power of combining and increasing those hereditary individual characters which they possess."

The views contained in this paragraph appear to me to be completely at variance with the facts known concerning the Fungi, among plants. Thus, in the Saprolegnieæ, all the known forms, including several genera and many species, are parthenogenetic; the sexuality of the Ascomycetes is still the subject of discussion, but it is admitted that many genera and species of these Fungi are certainly asexual; and the sexuality of the Æcidiomycetes is extremely doubtful. These plants show no apparent tendency to die out, in spite of the absence of sexuality. But it may be replied that these families may be in the stage in which sexuality is just disappearing, and in which they are still adequately adapted to their conditions of life. Such an objection cannot apply, however, to the Basidiomycetes. These Fungi are not only entirely asexual, but it would appear that they have been evolved in a purely asexual manner from asexual ascomycetous or Æcidiomycetous ancestors. The Basidiomycetes, in fact, afford an example of a vast family of plants, of the most varied form and habit, including hundreds of genera and species, in which, so far as minute and long-continued investigation has shown, there is not, and probably never has been, any trace of a sexual process. How are we, then, to account for all the variation which has taken place in this group, quite independently of amphigonic reproduction? On this point Prof. Weismann says (p. 275):—

> "If it could be shown that a purely parthenogenetic species had become transformed into a new one, such an observation would prove the existence of some force of transformation other than selective processes, for the new species could not have been produced by these latter."

It appears to me beyond doubt that, in the Fungi, new species have been developed from parthenogenetic forms, but I leave it to Prof. Weismann to suggest what "force of transformation other than selective processes" may have been operative.

It is not, however, argued that the variation of the higher Fungi is as great as it might have been had they possessed sexuality; for there can be no doubt that sexual reproduction does very materially promote variation. It seems probable, in fact, that the absence of sexuality in these plants may be just the reason why no higher forms have been evolved from them; for in this respect they present a striking contrast to the higher Algæ in which sexuality is well marked.

Since it is clear that new hereditary characters can be produced by the action of external influences on the germ, the outcome of Prof. Weismann's investigation of the phenomena of variation is that he has given prominence to the fact that new hereditary characters need not be apparent in the body of the parent, but that, on the contrary, the somatogenic characters are just those which are least likely to be transmitted. This is essentially the same position, though stated in more precise terms, as that taken up by Darwin, who held that it is not the sudden variations, due to altered external conditions, which become permanent, but those slowly produced by what he termed the accumulative action of changed conditions of life.

With this I close my criticisms, not because there are no other points which might be discussed, but because I have already touched upon many of them in my "Lectures on the Physiology of Plants" (Cambridge, 1886), and because I desire at present to deal solely with the more fundamental parts of the theory. I have, I think, said enough to show that, interesting and suggestive as is Prof. Weismann's theory of the continuity of the germ-plasm, it by no means affords, at least in its present form, so complete and ready an explanation of the facts of embryogeny, heredity, and variation, as the enthusiasm of some of his more ardent disciples would have us believe.

SYDNEY H. VINES.

Oxford, September.

NOTES.

WE regret to announce the death of Mr. John Ball, F.R.S., which took place somewhat suddenly at midnight on Monday last. We understand that the funeral will take place to-morrow (Friday), at 11 a.m., at St. Thomas's, Walham Green.

THE Reports of the Eclipse Expedition of 1886 are at length ready for publication, and will be issued immediately as separate numbers of the Philosophical Transactions. The first, "On the Total Solar Eclipse of August 29, 1886," is by Captain Darwin, Dr. Schuster, and Mr. Maunder; the second, "On the Observations made at the Island of Carriacou," is by the Rev. S. J. Perry; the third, "On the Determination of the Photometric Intensity of the Coronal Light," by Captain Abney and Prof. Thorpe; and the fourth, "On the Observations made at Grenville, in the Island of Grenada," by Mr. H. H. Turner.

THE collection of objects brought back by Prof. Haddon from various islands in Torres Straits is now to be seen in a part of the Eastern Assyrian room at the British Museum, on the upper floor of the north-east angle of the building. Special interest attaches to the anthropological specimens included in this valuable collection.

THE list of names to be recommended for the new Council of the London Mathematical Society at its annual meeting, on November 14, differs from that of last year in the following